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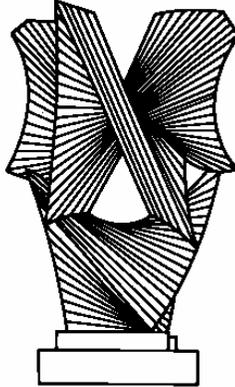
2006

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Recommended Citation

CHICAGO

REEFER MADNESS:
BROKEN WINDOWS POLICING AND MISDEMEANOR MARIJUANA ARRESTS IN NEW YORK CITY, 1989–2000

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THE LAW SCHOOL
THE UNIVERSITY OF CHICAGO

December 2006

Reefer Madness:
Broken Windows Policing and Misdemeanor Marijuana Arrests in New York City, 1989–2000.

Bernard E. Harcourt[1] and Jens Ludwig[2,3]

The pattern of misdemeanor marijuana arrests in New York City since the introduction of broken windows policing in 1994—nicely documented in Andrew Golub, Bruce Johnson, and Eloise Dunlap's article *The Race/Ethnicity Disparity in Misdemeanor Marijuana Arrests in New York City*—is almost enough to make an outside observer ask: Who thought of this idea in the first place? And what were they smoking?

By the year 2000, arrests on misdemeanor charges of smoking marijuana in public view (MPV) had reached a peak of 51,267 for the city, up 2,670 percent from 1,851 arrests in 1994. In 1993, the year before broken-windows policing was implemented, a New York City police precinct made, on average, 10 MPV arrests per year; by 2000, the police precincts were averaging 644 MPV arrests per year—almost 2 arrests per day per precinct. These misdemeanor MPV arrests accounted for 15 percent of *all* felony and misdemeanor arrests in New York City in 2000. That same year, New York City marijuana arrests represented 92 percent of the total 67,088 marijuana-related arrests in the State of New York.[4]

In addition, the pattern of arrests disproportionately targeted African-Americans and Hispanics in relation to their representation in the resident population. Although both

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[3] Special thanks to Andrew Golub for sharing the time series data on misdemeanor arrests for smoking marijuana in public view and for comments; to Stephen Schacht at NORC for comments and guidance; and to James Lindgren and Sherod Thaxton for comments and suggestions.

[4] Golub, Johnson, and Dunlap 2006:___.

groups each represent about 25 percent of New York City residents, they compose 52 and 32 percent of MPV arrestees for 2000-2003 respectively. African-American and Hispanic MPV arrestees have also fared worse in the criminal justice system: they were more likely than their white counterparts to be detained before arraignment (2.66 and 1.85 times more likely, respectively), convicted (both twice as likely) and sentenced to additional jail time (4 and 3 times more likely, respectively).[5] In a city in which tensions between the police and the minority community were already running high as a result of (potentially productive) NYPD efforts targeted at guns and serious violent crime, stopping minority residents at disproportionately high rates for smoking marijuana in public seemed unlikely to do much to ease this friction.

We have reviewed and analyzed the MPV arrest data and have only one thing to add: In addition to imposing costs disproportionately on New York City's minority residents, there is no good evidence that this "reefer madness" policing strategy contributed to the decline in the sorts of serious crimes that are of greatest public concern in New York City. In order to justify the substantial race disparity in marijuana arrests, the NYPD must believe that some important social objective is being accomplished. This larger objective is presumably not reducing marijuana consumption per se, and seems more likely to be the intention of reducing more serious offenses under the standard "broken windows" argument articulated nearly 25 years ago by James Q. Wilson and George Kelling.[6] Perhaps the belief that this policing strategy can reduce serious crime might also stem from the hypothesized link between drug markets and violence, even though most criminologists believe that violence is much less common in the market for marijuana than that for, say, crack cocaine. The psychopharmacological effects of marijuana use on criminal or violent behavior are also believed to be much less pronounced than with many other commonly-used drugs, including alcohol.

In any case, whatever the conceptual underpinning of this marijuana policing strategy, we have analyzed the MPV arrests building on our previous research on broken windows policing[7] and, using a number of different statistical approaches on these MPV arrest data, we find no good evidence that the MPV arrests are associated with reductions

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[5] Golub, Johnson, and Dunlap 2006:___.
[6] Wilson and Kelling 1982.
[7] Harcourt and Ludwig 2006.

in serious violent or property crimes in the city. As a result New York City's marijuana policing strategy seems likely to simply divert scarce police resources away from more effective approaches that research suggests is capable of reducing real crime.[8]

The policy recommendations that Golub, Johnson, and Dunlap make—especially reducing the intensity of MPV patrolling and making the MPV charge a violation rather than a misdemeanor—seem consistent with two of the primary goals that should animate any major metropolitan police department, namely crime control and fairness. One other reform that should be added to the list concerns the legal standard of review in cases involving such pronounced racial or ethnic disparities in the criminal justice system: Courts reviewing claims of racial or ethnic discrimination in policing, where the *prima facie* evidence of discrimination cuts across several layers of outcomes (arrest, detention, conviction, and additional incarceration) should relax the requirement that the complainant prove actual discriminatory intent on the part of a particular actor, and instead allow for an inference of intent where the government has failed to justify or explain a number of those disparities.[9] This change would effectively introduce a *Batson*-type analysis in court review of claims of police discrimination and shift the burden of explaining gross disparities on the party with the most complete information—in this case, the NYPD.

I.   The Effects of Policing Public Marijuana Smoking on Crime

At our request, Andrew Golub generously shared the time series data on misdemeanor MPV arrests in New York City from 1980 to 2003. We merged these records with a dataset we had put together previously for research on broken-windows policing—data which we analyzed in our article *Broken Windows: New Evidence from New York City and A Five-City Social Experiment* published in the University of Chicago Law Review in 2006. We discuss our data collection in an appendix to this study, but here move directly to the results of our statistical analyses.

At first glance a standard panel-data analysis seems to provide some support for the belief that stepped-up enforcement of MPV offenses contributes to a decline in more

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[8] For a review of those approaches, *see* Sherman, 2002; Cohen and Ludwig, 2003.

[9] For an argument to this effect in the context of racial profiling more generally, *see* Harcourt 2004: 1346—1354.

serious offenses. As in our earlier study published in the University of Chicago Law Review, which re-examined and ultimately rejected Kelling and Sousa's (2001) claim that broken windows policing was a major driver for the crime drop in New York City, we use repeated cross-sections for the city's 75 police precincts over the course of the 1990s. But now instead of relating precinct counts for serious offenses to overall misdemeanor arrests, we focus more narrowly on misdemeanor MPV arrests to test the hypothesis that focused anti-pot enforcement might be more effective than a more general "broken windows" misdemeanor strategy. Our specific estimating equation is as follows:

(1) $CRIME_{py} = \alpha + \beta\ MPV\ ARRESTS_{py} + \theta\ CONTROLS_{py} + \gamma_p + \delta_y + \varepsilon_{py}$

where p represents precincts and y reflects the year. Our initial dependent variable of interest is the annual precinct violent crime count, which we obtained by aggregating the annual precinct counts for murder, robbery, rape and aggravated assault. The annual MPV arrest numbers is the key explanatory variable of interest. Our model also conditions on precinct and year fixed effects (γ_p and δ_y) to account for unmeasured factors that influence crime and are either constant within precincts over our study period, or change over time but exert a constant influence over the entire set of city precincts. The model also includes a standard set of control variables described in Table 1 and in more detail in Harcourt and Ludwig (2006); we do not spend much time discussing their estimated impacts given space constraints. We account for arbitrary forms of correlation in our models' error structure by calculating robust standard errors that are clustered at the level of the police precinct.

        The results from this first cut on the data, shown in Table 1, suggest that the annual precinct counts of MPV arrests have a significant negative effect on our index of violent crime, and that this relationship remains negative using different models. The main association is qualitatively similar when we change the set of covariates included in the model, focus on lagged rather than contemporaneous values of the MPV arrest variable, or estimate a model in logs rather than levels.

TABLE 1

Panel Data Analysis of the Effects of Policing Marijuana MPV on Violent Crime
Dependent variable = annual precinct violent crime count

Explanatory variables:	Model 1	Model 2	Model 3	Model 4
MPV arrests	−0.630	−0.619	−0.540	−0.353
	[0.124]**	[0.128]**	[0.115]**	[0.110]**
NYPD Manpower		−0.726	−0.706	−2.219
		[1.198]	[1.138]	[1.179]
Percent Black			34.155	54.610
			[10.073]**	[14.935]**
Percent Hispanic			42.853	53.524
			[17.161]*	[22.462]*
Precinct Population				0.010
				[0.004]*
Precinct and year fixed effects?	Yes	Yes	Yes	Yes
Control for unemployment, drugs, and proportion 19 to 24?	No	Yes	Yes	Yes
Control for other covariates?	No	No	No	Yes
N	900	888	888	888
R-squared	0.90	0.91	0.92	0.94

Robust standard errors in brackets
* =   statistically significant at 5% cut-off
** =   statistically significant at 1% cut-off

The trouble with this standard panel-data setup is that it ignores mean reversion. Any study of crime patterns during the 1990s has to take account of the massive period effects on crime during the 1980s and 1990s. The dramatic increase in crime rates observed in places like New York City and elsewhere from the mid 1980s through the early-to-mid 1990s is thought to have been driven largely by the growth in crack cocaine use and involvement of firearms in the new street markets for crack.[10] Using city-level data, Steven Raphael and Jens Ludwig have shown that those cities that experienced the largest increases in crime during this period subsequently also experienced the largest crime drops.[11] This is consistent with Steven Levitt's (2004) hypothesis that the ebbing of the crack epidemic is one of the four important contributors to the American crime drop in

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[10] *See* Blumstein 1995:10 (examining some empirical data reflecting changing crime patterns beginning in the mid 1980s and concluding that the illegal drug markets' recruitment of youths resulted in a dramatic growth in youth homicide); Cook and Laub 2001:22 discussing epidemics of youth violence in different time periods and concluding that the observed youth violence of the late 1980s was closely tied to the epidemic of crack cocaine).

[11] *See*   Raphael and Ludwig 2003:267 ("To summarize, the large increase in homicide rates occurring during the late 1980s in Richmond coupled with the inverse relationship between earlier and later changes in homicide rates observed among other U.S. cities casts doubt on the validity of previous claims about the effects of Project Exile.").

the 1990s (the others being increased incarceration and spending on police, and abortion legalization in the early 1970s).[12] We would expect places that were hit hardest by crack to show the largest subsequent declines in crime when crack's impact begins to dissipate.

A natural concern is that mean reversion may be at work at the police precinct level in New York City as well, a possibility that receives support from Figure 1: MPV enforcement was most intense within the New York neighborhoods where we would expect mean reversion to be most pronounced during the 1990s. Specifically, Panel A shows that in 1989 precincts with higher violent crime also have higher MPV arrests. That is, the regression line relating violent crime and MPV arrests in 1989 has a positive slope. Panel B shows that the most violent precincts in 1989 also experienced the largest *increase* in MPV arrests from 1989 to 2000. Panel C shows that the neighborhoods with the highest violent crime in 1989 experienced the largest declines in violent crime from 1989 to 2000.

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[12] Steven D. Levitt (2004) Understanding Why Crime Fell in the 1990s: Four Reasons that Explain the Decline and Six that Do Not." *Journal of Economic Perspectives*. 18(1): 163-190.

FIGURE 1

MPV Arrests and Violent Crime
in NYC Precincts, 1989–2000
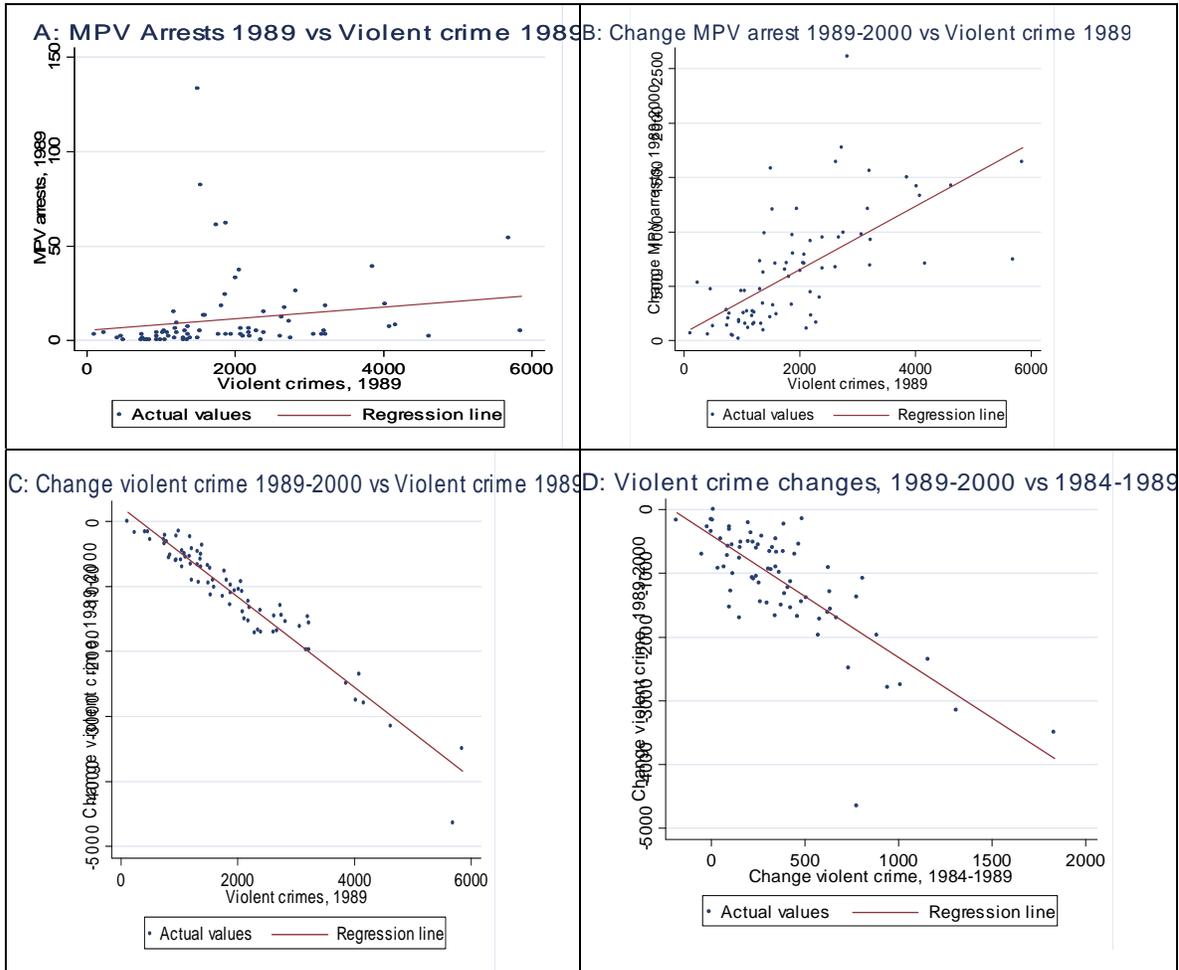
Why do precincts with unusually high initial crime rates experience unusually large declines in crime thereafter? Mean reversion seems to be an important explanation. Panel D shows that, as is true with city-level crime data, those police precincts with the largest increases in crime during the crack epidemic have the largest declines thereafter.

We can illustrate the basic idea somewhat more formally by estimating a first-difference model that relates changes across precincts from 1989 to 2000 in precinct violent crime to changes over this period in precinct MPV misdemeanor arrests, controlling for other changes in explanatory variables. One advantage of this specification over the standard panel-data setup as in equation (1) is to allow for a very

straightforward way to control for the possibility of mean reversion, by explicitly conditioning on the magnitude of each precinct's increase in violent crime during the crack epidemic.[13] The basic estimating equation is as follows:

(2) $\Delta\ CRIME_p = \alpha + \beta\ \Delta\ MPV\ ARRESTS_p + \theta\ \Delta\ CONTROLS_p + \varepsilon_p$

The results of this first-difference analysis, reported in Table 2, reveals that the change in MPV arrests only has a statistically significant negative effect on changes in violent crimes when no other control variables are included in the model. As soon as we add a variable that helps capture mean reversion (the increase in crime for each precinct through the height of the crack epidemic), the coefficient turns positive and remains statistically significant under different model specifications—adding, for example, another control for mean reversion, controls for three other explanations for the crime drop of the 1990s (drug use patterns, unemployment, and youth demographics), a control variable for the NYPD manpower change, and changes in the proportion Hispanic and African-American.

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[13] For general discussion of mean reversion, *see* Raphael and Ludwig, 2003; Harcourt and Ludwig, 2006.

Table 2

Regressing Violent Crime Changes against MPV Arrest Changes
Dependent variable = Precinct change in violent crime, 1989–2000

Explanatory variables:	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Change in MPV Arrests 1989–2000	–0.864	0.255	0.270	0.181	0.182	0.159
	[0.159]**	[0.061]**	[0.059]**	[0.061]**	[0.061]**	[0.058]**
Violent Crime 1989		–0.767	–0.843	–0.797	–0.778	–0.763
		[0.027]**	[0.040]**	[0.034]**	[0.038]**	[0.036]**
Change Violent Crime 1984–89			0.306	0.100	0.070	–0.011
			[0.124]*	[0.108]	[0.113]	[0.111]
Change Manpower 1989–2000				1.870	2.113	1.791
				[0.799]*	[0.806]*	[0.770]*
Change Percent Black 1989–2000					4.525	6.021
					[2.448]	[2.369]*
Change Percent Hispanic 1989–2000					–0.509	–4.244
					[4.765]	[4.681]
Change in non-MPV misdemeanor arrests 1989–2000						0.056
						[0.019]**
Control for change in drugs, unemployment, and youth population 1989–2000	No	No	No	Yes	Yes	Yes
N	75	75	74	73	73	73
R-squared	0.29	0.94	0.95	0.97	0.97	0.97

Robust standard errors in brackets. Models 3 through 6 exclude NYPD precinct 49, because we have no crime data for that precinct for 1984; Models 4 though 6 exclude NYPD precinct 22 (Central Park) because there are no controls for drugs, unemployment and youth population.
* = 　　　statistically significant at 5% cut-off
** = 　　statistically significant at 1% cut-off

The positive relationship between the change in MPV arrests and serious crime, when prior crime levels is held constant, means that, controlling for mean reversion, an increase in MPV arrests over the period translates into an *increase* in serious crime—not, as the broken windows theory would predict, a decrease in serious crime. This is exactly the opposite of what we would want in terms of the effect of MPV arrests. It suggests that this policing strategy focused on misdemeanor MPV arrests is having exactly the wrong effect on serious crime—*increasing* it, rather than decreasing it.

What Table 2 thus reveals is the important role of mean reversion when analyzing crime data from the 1990s. In our data, the precincts that received the most intensive broken windows policing during the 1990s, as measured by MPV misdemeanor arrests, are the ones that experienced the largest increases in crime during the city's crack epidemic of the mid-to-late 1980s. Consistent with findings elsewhere from city-level data,[14] jurisdictions with the greatest increases in crime during the 1980s tend to experience the largest subsequent declines as well. We have called this "Newton's Law of Crime"[15] and see it again at work here: what goes up must come down (and what goes up the most tends to come down the most).

The final column of Table 2 reveals that, in a "horse race" comparison of the effect of changes in misdemeanor MPV arrest rates and non-MPV misdemeanor arrest rates, both are positively related and statistically significant—though the effect of MPV arrest rates on crime is much larger.

These conclusions are, overall, consistent with our earlier statistical findings concerning the effect of total misdemeanor arrests on serious crime in New York City, presented in *Broken Windows*.[16] In that research, we used a similar approach to analyze the relationship between changes in total misdemeanor arrests within New York City precincts from 1989 to 1998 and changes in the violent crime rate. We found that, if anything, increases in misdemeanor arrests were accompanied by *increases* in violent crime. While the positive relationship between changes in misdemeanor arrests and changes in violent crime was somewhat sensitive to the model specification, there was no evidence from that first-difference model of a *negative* relationship between changes in total misdemeanor arrests and violent crime. We concluded there that the evidence, as shown in our original Table 3 in Harcourt and Ludwig 2006, was not consistent with the idea that stepped-up zero-tolerance policing reduces crime. We reproduce here Table 3 from that study.

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[14] See Raphael and Ludwig 2003: 265 (positing that the reduction in violence in such areas finds its root, not in federalized prosecution of eligible gun offenses, but rather in the fact that the violence accompanying the introduction of crack cocaine in the 1980s had run its course by the late 1990s).

[15] Harcourt and Ludwig 2006: 276.

[16] Harcourt and Ludwig 2006.

TABLE 3 FROM HARCOURT AND LUDWIG 2006

The Effects of Model Specification and Mean Reversion in the Kelling-Sousa Analysis:
Regressing Crime Changes against Arrest Changes

Explanatory variables:	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Change misdemeanor arrests, 1989–98	–086 (.074)	.046 (.051)	.114** (.022)	.114** (.022)	.094** (.025)	.004 (.030)
Violent crime, 1989			–.660** (.023)	–.710** (.039)	–.716** (.039)	–.625** (.041)
Change violent crimes, 1984–89		-1.762** (.183)		.214 (.133)	.243* (.137)	–.013 (.127)
Change manpower, 1989–98					1.412 (.963)	3.326** (1.065)
Other covariates?	N	N	N	N	N	Y
N	75	74	74	74	74	74
R-squared	.018	.561	.924	.926	.928	.969

Dependent variable = Precinct change violent crimes, 1989–1998. Other covariates include change from 1989 to 1998 in poverty, racial and age composition of the population, percent households headed by females, public assistance, and vacant housing.
* = Statistically significant at 10 percent cut-off.
** = Statistically significant at 5 percent cut-off.

NB: The table as originally published in the *University of Chicago Law Review* contains errata concerning the signs of the coefficients in the first and third rows of the table. The values here are correct.

II. Shifting the Burden of Proof Where Such Strong Evidence of Racial Disparities Exists

The policy recommendations advanced by Golub, Johnson, and Dunlap seem appropriate, especially in light of our further findings. We would add just one important suggestion that would place the burden of explaining the impact of public policies in cases like this—where there is such strong *prima facie* evidence of disparate racial and ethnic impact across a range of criminal justice outcomes—on the agency with the most information: courts especially, but legislative bodies as well, should shift the burden of proof onto governmental agencies when there is strong facial evidence of discrimination. In effect, courts should introduce a *Batson*-type analysis in reviewing claims of intentional discrimination in policing. This could be done either through the judicial adoption of a *Batson*-framework or by legislative action.

As a technical constitutional matter, under the Fourteenth Amendment as presently interpreted, any claim of discrimination against the NYPD for the disparity in MPV arrests would require a showing of intent on the part of the police officers or department. For a legal challenge to withstand scrutiny, a complainant would need to establish invidious intent by an actor—either individual police officers or the administrators and policy makers at the NYPD. The fact is, the mere existence of a disparity does not prove intent. A disproportionate impact on minorities does not, standing alone, mean that the NYPD has engaged in invidious racial discrimination. It does not exclude the possibility that the NYPD has been pursuing a legitimate end: either pursuing all MPV offenders (and they are distributed unevenly) or even using race or ethnicity as a proxy for higher risk.

It is precisely for this reason that we do not know whether the disparities reflect the intentional use of race or ethnicity in policing in New York City. Golub, Johnson, and Dunlap are careful not to claim intentional discrimination, precisely because they have no data on real offending rates for MPV, nor do they have sufficient data on the background characteristics of the arrestees to compare their criminal justice outcomes. Not knowing the exact criminal record of each person arrested for an MPV offense, it is impossible to hold constant prior criminality in the regressions on criminal justice outcomes.

The evidence of disparate impact at several stages of criminal justice outcomes (from arrest through incarceration) is strong enough here, however, that instead of requiring a complainant to prove intent—which is really an impossible standard to meet—the analysis of any Equal Protection challenge should follow the three-step model articulated by the Supreme Court in the case of *Batson v. Kentucky*, which dealt with challenges to the racial composition of a prosecutor's peremptory strikes of potential jurors. Adopting a *Batson* framework would not eliminate the intent requirement; rather, it would merely extend the *Batson* method of inferring intent to the policing context.

Under a *Batson*-type approach, significant statistical discrepancies in the race of persons arrested, detained, convicted, and sentenced would satisfy the first prong of the analysis and set forth a *prima facie* case. This would shift the burden to the governmental agency to then explain the reason for the disparities. In this case, the police department or units would then be required either to offer race-neutral reasons for the disparities—that

is, to offer other factors that, when held constant, eliminate the racial correlation with arrests—or to present evidence that race is a statistically significant predictor of serious crime and that profiling satisfies the limited conditions that make it constitutionally acceptable to use race—namely, that it is narrowly tailored to a compelling state interest.[17] If the state satisfies its burden, then the challenging party should have the opportunity to rebut the state's evidence.

Over the spectrum of policing initiatives, the NYPD may have legitimate reasons to engage in policing interventions that have disparate impact on racial or ethnic groups as compared to their representation in the resident population. It may be the case, for instance, that a racial or ethnic group represents a higher proportion of the offending population than it does the resident population. Or it may be that other legitimate characteristics proxy on race or ethnicity. Disparate impact is not, in itself, prohibited. But where there is such strong evidence of disparate impact, the burden should be on the agency with the information to explain what is causing the imbalance.

What our findings do add to this analysis is that they would preclude the NYPD from arguing that profiling Hispanic and African-American residents in the MPV context is narrowly tailored to the compelling state interest of combating serious crime. Even though this may be an interest that satisfies equal protection analysis in come cases, there is no evidence that the broken windows MPV strategy has had the desired effect on serious crime.

III.   Conclusion

New York City's psychedelic experiment with misdemeanor MPV arrests—along with all the associated detentions, convictions, and additional incarcerations—represent a tremendously expensive policing intervention. As Golub, Johnson, and Dunlap document well, the focus on MPV has had a significant disparate impact on African-American and Hispanic residents. Our study further shows that there is no good evidence that it contributed to combating serious crime in the city. If anything, it has had the reverse effect. As a result, the NYPD policy of misdemeanor MPV arrests represents an

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[17] There is some controversy over whether combating serious crime amounts to a compelling state interest that would allow the police to use race explicitly in policing. *See* Harcourt 2004:1349 n.184. I assume here that it would, especially if the crime is serious.

extremely poor trade-off of scarce law enforcement resources, imposing significant opportunity costs on society in light of the growing body of empirical research that highlights policing approaches that do appear to be successful in reducing serious crime.[18] Our findings, building on those of Golub, Johnson, and Dunlap, make clear that these are not trade-offs in which we should be engaging.

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[18] *See generally* Sherman, 2002; Cohen and Ludwig, 2003.

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Appendix: Data Collection

At our request, Andrew Golub shared with us the time series data on MPV arrests in New York City, for which we are deeply grateful. The rest of the data were assembled for our earlier study, *Broken Windows: New Evidence from New York City and A Five-City Social Experiment* (2006). We obtained New York City crime and other arrest data for our key dependent and explanatory variables directly from the New York City Police Department (NYPD). To measure violent crime, we use precinct-level reports of four violent offenses (murder, rape, felonious assault, and robbery), though we also have individual measures for these and other Part I offenses. We have these data from 1989 through 2000. We also have precinct-level reports for other types of crime, including property offenses.

There were 75 NYPD precincts in 1989 and there are 76 NYPD precincts today. precinct 34 was divided in two in 1994, creating NYPD precinct 33. We have merged data from those two precincts (33 and 34) back together to recreate the original 75 precincts in order to compare them over the full time period. In Table 2, Models 3 through 6 exclude NYPD precinct 49, because we have no crime data for that precinct for 1984, thus making it impossible to calculate the increase in crime from 1984 to 1989 for purposes of testing mean reversion; Models 4 though 6 exclude NYPD precinct 22 (Central Park) because there are no controls for drugs, unemployment and youth population.

We decided to use counts rather than rates because the residential populations in the precincts to not correspond well with day-time populations. It is worth noting, though, that our results are not sensitive to decisions about whether to weight by precinct population or not, or to work in per capita crime and arrest rates rather than counts. In terms of residential populations, excluding the Central Park precinct, precinct populations vary between 16,179 and 242, 948, with a mean of 103,402. These numbers, however, do not reflect day-time populations. So, for example, NYPD precinct 14 has the lowest residential population—16,179 in 2000—in part because it is the Midtown South precinct that covers Time Square and the Garment District, primarily a commercial and entertainment oriented precinct. It turns out, though, that the 14th precinct has a lot of

MPV arrests. In 2000, it ranked 24th (out of 75 precincts) in terms of MPV arrests, with 795 arrests. Using a population weight here would clearly distort the result. The same is true for the next smallest precinct, NYPD precinct 1 in Manhattan, which covers City Hall and the Wall Street area, as well as NYPD precinct 22, the Central Park precinct. Residential population numbers here are simply inapposite. Since the residential population numbers are not necessarily related to day-time population numbers, it is more conservative to use counts rather than rates.

One challenge for our study is that data on important potential confounding factors are not readily available for New York City at the precinct level. To proxy the effect of cocaine-related drug consumption, we obtained borough-level data on hospital discharges for drug-related causes from the New York State Department of Health, Bureau of Biometrics, and extracted reports of hospital discharges for cocaine-related episodes. To measure unemployment, we have obtained borough-level data on the annual average number of unemployed persons from the New York State Department of Labor. Whether data measured at the level of New York's five boroughs adequately captures variation in social and policy conditions across the city's seventy-six separate precincts is an open question. Moreover, the hospital discharge data by its nature cannot distinguish between the prevalence of crack use and powdered cocaine consumption. The standard concern in the case of poorly measured explanatory variables is attenuation—bias towards zero in the coefficients for these covariates.

In addition, we have incorporated census tract-level measures of racial and ethnic composition and age distribution, taken from the 1990 and 2000 decennial censuses. Data for the intercensal years are linearly interpolated. Because census tract and police precinct boundaries do not perfectly overlap in New York City, we have geocoded both tract and precinct boundaries, and then aggregated tracts up to the precinct level by assuming that the population of tracts that cross precinct boundaries are distributed across precincts proportionately to the tract's land area.[19] We use these census data to calculate measures of each precinct's distribution of youths (19 to 24) and racial and ethnic composition.

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[19] Suppose for example that census tract 1 lies entirely within precinct A, tract 2 lies entirely within precinct B, but 25 percent of the land area of tract 3 is in precinct A while 75 percent of the land area of tract 3 is within precinct B. Let X_i be some population characteristic for tract (i), such as percent poor, and let P_i represent the population of tract (i). In this case we calculate percent population poor in precinct A as $(P_1 \times X_1 + (0.25)P_3 \times X_3)/(P_1 + (0.25)P_3)$.

We have also included, using the same method, other covariates consisting of measures of each precinct's age distribution, poverty rate, female-headed households, fraction of adults with different levels of educational attainment, median income, and welfare receipt. To measure physical signs of disorder we control for the fraction of housing units in the precinct that are vacant. These measures capture structural disadvantage (percent of the precinct that is poor, receiving public assistance, or has less than a high school degree), demographics (percent of the precinct in their peak offending ages, percent of households headed by a female, percent black), and measures of physical disorder (percent of housing units that are vacant).

Finally, we also incorporated into our dataset a measure of the number of police officers assigned to each precinct in each year by the NYPD. One important conceptual concern is whether its key explanatory variable of interest—the misdemeanor arrest rate—captures the effects of changes in how police resources are deployed or instead simply reflects increased police presence. This explanation is of some concern because, from 1994 to 1998 the size of the NYPD force increased by about a half.[20]

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[20] See Harcourt 2001:94–95. The police manpower variable is potentially problematic because some arrests within a precinct might be made by law enforcement officers who are officially assigned to different areas, although our results are not sensitive to excluding this variable.